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Feature priming in visual search does not depend on the dimensional context

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Visual search is speeded when the target-defining property (a feature- or dimension difference relative to the distractors) is repeated relative to when it changes, a phenomenon referred to as intertrial priming. Feature priming is usually weaker than dimension priming, and sometimes even absent. Four experiments tested the hypothesis that feature priming effects are especially weakened when the visual search task also involves dimension changes, the idea being that feature changes become less salient or less relevant relative to the bigger dimension changes. Feature changes were embedded in blocks that only contained feature changes, or in blocks that also carried dimension changes. However, regardless of type of search task, and level of display ambiguity, the dimensional context had little to no effect on the magnitude of feature priming. There were only reliable effects of ambiguity, in line with recent proposals (Meeter & Olivers, 2006).

Human attention is subject to implicit mechanisms that, without the observer's awareness, continuously change the perceptual system's sensitivity to an altered environment. An example of this is the phenomenon of priming in visual search. In a typical visual search task, the observer is asked to look for, and respond to, a target object that differs on a certain property (e.g., colour, orientation, shape, or a particular combination of these) from a set of spatially dispersed distractor objects. It has now been found in many studies that when the target-defining feature is uncertain from trial to trial, repeating the target feature results in better performance (see Fecteau & Munoz, 2003, for an overview). For example, as shown by Maljkovic and Nakayama (1994), a red target following a red target (i.e., a repeat trial) leads to faster response times (RTs) than a red target following a green target (i.e., a switch trial). We will refer to these effects as intertrial effects.

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Intertrial effects occur not only on a feature level, but also on a dimension level. For example, Müller, Heller, and Ziegler (1995) found that RTs increase when targets change from being colour-defined to being orientation-defined, or vice versa, relative to when dimensions are repeated. In fact, dimension-based intertrial effects are often found to be much stronger and more reliable than feature-based intertrial effects (Found & Müller, 1996; Müller, Krummenacher, & Heller, 2004; Müller, Reimann, & Krummenacher, 2003; Olivers & Humphreys, 2003; Olivers & Meeter, 2006; Weidner, Pollmann, Müller, & von Cramon, 2002; see also p. 14 and Note 4 of Müller et al., 1995). Intuitively, this is not surprising, given that a dimension switch embodies a larger perceptual change than a feature switch. Furthermore, across the board, the visual system is more interested in relative than in absolute values. What makes red or green targets salient is not that they are green or red per se, but that they stand out against the background of distractors on the colour dimension. In other words, exact feature information is often less available or less relevant than dimension information (e.g., Kumada, 1999; Müller et al., 1995).

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What is more remarkable is that whereas many have found weak and unreliable feature-based intertrial effects, some have found such effects to be rather strong and robust (Bichot & Schall, 2002; Goolsby & Suzuki, 2001; Hillstrom, 2000; Kristjánsson, Wang, & Nakayama, 2002; Maljkovic & Nakayama, 1994; McPeek, Maljkovic, & Nakayama, 1999). Recently, we have identified one factor modulating the occurrence and strength of intertrial effects, whether feature- or dimension-based (Meeter & Olivers, 2006; Olivers & Meeter, 2006). We found that intertrial effects depend on the level of ambiguity in the visual search task. Ambiguity refers to the situation in which there is the opportunity for confusion as to what the target is. For example, such confusion may arise from the fact that selection settings are generally fuzzy in nature. Imagine the task is to search for a singleton target that can be either red or green. Together with others (Müller et al., 2003), we propose that in such tasks the attentional set will not be very precise—that is, it will not be confined solely to red or green, but is likely to be set to some extent for colour singletons in general, and perhaps even singletons in general (as defined on a different dimension than just colour). The assumption is that at each level, the system can only be set for one type of information at a time (i.e., one colour and one dimension). That is, the system can be set for red, but when red is then not available (because the target has changed to green), it looks for colour in general instead. This is not problematic when the target is the only singleton in the display, since

even the wider defined attentional set will only lead to selection of the target. Hence, target repetitions or switches will have little effect. However, when the target is not the only singleton in the display (as it is accompanied by a singleton distractor), and at the same time the target colour switches, confusion may arise as to which of the two items should be responded to, since both objects match the general selection settings. Both items will be selected, and task rules need to be retrieved to decide which of the two objects is the correct one, a process that results in delays.

Ambiguity may not only be caused by the stimulus. Olivers and Meeter (2006) found that the influence of ambiguity could also play out on the task level: Intertrial effects occurred in search tasks on which the presence of a target was uncertain and participants made a present/absent decision, but not in tasks in which a target was always present and a participants decided on one of its features (the latter task is often referred to as a compound search task)—despite the displays being exactly the same under these different task conditions. We argued that the present/absent task contains more uncertainty (read: Ambiguity) than the compound task. Note here that these effects cannot be explained simply by increased task difficulty, since discrimination tasks (such as the compound task) are usually perceived as more difficult than detection tasks (such as the present/absent task). Furthermore, Meeter and Olivers (2006) showed that merely making the task more difficult, by reducing the size of the search items, resulted in overall increased RTs, but not in increased intertrial effects.

Dimensional context

In the present study, we investigate another factor that might affect the strength of intertrial effects—more specifically feature-based intertrial effects. A review of the literature reveals that in those studies that found strong feature-based intertrial effects, there were only feature-based changes (e.g., between red and green; Bichot & Schall, 2002; Goolsby & Suzuki, 2001; Hillstrom, 2000; Kristjánsson et al., 2002; Maljkovic & Nakayama, 1994; McPeek et al., 1999), whereas in those studies that failed to find (strong) feature-based effects, the feature-based changes were randomly mixed with dimension-based changes (e.g., between colour and orientation, Found & Müller, 1996; Müller et al., 1995, 2003, 2004; Olivers & Humphreys, 2003; Olivers & Meeter, 2006; Weidner et al., 2002).

This raises the possibility that the occurrence of feature-based effects depends on the trial context. For example, feature changes may become relatively less salient when intermixed with the perceptually larger dimension changes, and therefore attract fewer attentional resources than when they would occur in isolation.

Alternatively, top-down processes may assign fewer attentional resources to feature changes, when such resources are already required to deal with dimension changes. Within the dimension-weighting account of Müller and colleagues (Müller et al., 1995; Müller & Krummenacher, 2006), it is assumed that dimension changes are accompanied by the necessity to shift attentional weights from one dimension to the other. Within a dimension, weights may then be further fine-tuned towards a specific feature. Müller and colleagues say little about feature effects, mainly because such effects tended to be small. However, we could think of a version of dimension weighting in which there are fewer resources left for feature differentiation when dimension discrimination is also required. Furthermore, assuming that weight shifting takes time, one could imagine such shifting to occur more rapidly when they only occur between features than when first a shift between dimensions is required. For example, when the target has just switched from left-tilted to red, the system may have had sufficient time to shift weights towards the colour dimension before the next trial started, but not enough time to also fine-tune the weights towards redness.

On a related view, feature changes may simply become less relevant when they are surrounded by dimension changes: Observers may decide to employ an attentional set for general colour differences and orientation differences, without putting any additional effort into trying to distinguish specific colours or orientation. In contrast, when there are only feature-based changes, observers may deem it useful to optimize selection on a feature level. Such different attentional sets or "search modes" have been suggested before in a more spatial rather than a temporal context (Bacon & Egeth, 1994): Observers can adopt a singleton detection mode (looking for general differences) or a feature search mode (looking for specific features) depending on the nature of the task and the displays at hand. There is currently some debate as to what extent singleton and feature search modes interact with intertrial priming (Lamy, Bar-Anan, Egeth, & Carmel, 2006; Lamy, Carmel, Egeth, & Leber, 2006; Pinto, Olivers, & Theeuwes, 2005).

Thus, there are several scenarios under which feature changes should exert stronger effects when presented in isolation than when presented in a context of more salient or more relevant dimension changes. Four experiments tested the hypothesis that the trial context affects the strength of feature-based intertrial effects. We compared the magnitude of feature-based intertrial priming effects in *feature context* blocks (in which, if any, only feature changes could occur) to those in *dimension context* blocks (in which also dimension changes occurred). Experiment 1 revealed no difference: Feature-based intertrial effects were small and did not change depending on context. Since effects were small in the first place, Experiments 2 and 3 served to strengthen intertrial effects by increasing display ambiguity (following Meeter & Olivers, 2006; and Olivers & Meeter, 2006).

In Experiment 2, this was done by increasing display heterogeneity, whereas in Experiment 3, a salient distractor was added to the search set. Furthermore, Experiment 3 employed a compound search task instead of a present/absent search task, to see if the results depended on the type of task. Finally, in Experiment 4, dimension changes were removed to see if any absence of a difference was due to carry-over effects from one context to the other. None of these manipulations changed the main result, namely that feature-based intertrial effects remained largely equal across dimension context and feature context blocks. We conclude that the dimensional context is not a contributing factor to feature-based intertrial effects.

EXPERIMENT 1

In this first experiment, participants determined whether a target bar was present or absent in a field of grey vertical distractor bars. The target was either colour-defined (in which case it was either red or green) or orientation-defined (in which case it was either left-tilted or right-tilted). From trial to trial, the target could either remain the same, or change. In the feature context blocks, the target would change only within a certain dimension (between red and green in a colour block, and between left-tilted and right-tilted in an orientation block). In the dimension context blocks, targets could not only change within a dimension, but also between dimensions (from being colour defined to orientation defined and vice versa).

The hypothesis that the dimensional context affects feature-based intertrial effects would predict such effects to be increased in feature context blocks relative to the dimension context blocks. To maximize the potential effect of context, in one type of dimension context block, the majority of changes were dimension changes (i.e., 50% of the target present trials; 25% of the trials contained feature changes, and the remaining 25% of trials were repetitions). Note that this way there were actually fewer repetitions than in the feature-change only blocks (25% vs. 50%). To control for the possibility that any effects would be due to this change in proportion, we also included a dimension context condition in which there were 50% repetitions, 25% feature changes, and 25% dimension changes.

Method

Participants. Eighteen observers participated (eight female, mean age 22.0 years, range 17–33), all having self-reported normal eyesight and colour vision. Two additional observers took part but made more than 30% errors in one of the conditions. They were excluded from the analyses.

Stimuli and apparatus. All stimulus presentations and event timings were done using E-Prime (Psychological Software Tools, Inc.), running on Compaq Pentium IV machines. Search displays consisted of an 11×9 grid filled with line elements, covering a 19.4×19.4 degree square of the visual field on a 19-inch monitor running in 800×600 SVGA mode, viewed from approximately 70 cm in a dimly lit room. The locations of the elements were slightly jittered with respect to the grid, by introducing continuously distributed noise in position with a spread of 0.5 degrees. All line elements except the target were grey (12.0 cd/m^2), filled, upright bars of 2.1×1.1 degrees. The target, when present, was a unique bar placed on one of 12 grid positions that surrounded the centre of fixation at a radius of about 3.5 visual angle. Targets were the same size as the nontarget elements, but were either coloured red or green (CIE $x = 0.597$, $y = 0.366$, luminance 9.51 cd/m^2 and CIE $x = 0.254$, $y = 0.649$, luminance 12.13 cd/m^2, respectively), or tilted left or right by 30 degrees.

Procedure. Each participant completed four tasks. In all tasks, trials began with a 500 ms presentation of a fixation cross. Then the search display was presented until the observer had made a response as to whether a target was present or absent. The "m" key was pressed with the right index finger when the target was present; the "z" key was pressed with the left hand when the target was absent. Observers received visual feedback when they had made an error, and after each block they were informed of their mean reaction time and error rate. Participants first received a block of 32 practice trials, in which all trial types were mixed. They then received 24 blocks of 48 trials each. There were eight *feature-context* blocks. Four of these were colour-context blocks, in which the target was upright and could only change between red and green. There were 25% absent trials. Of the remaining 75% of trials, there were 50% colour changes and 50% repetitions. The same was true for four orientation-context blocks, except that in these blocks the target was always grey, but its orientation could change between left-tilted and right-tilted. There were eight *dimension context-50* blocks. In these blocks, the target could not only change within a dimension, but also between dimensions. It did so on 50% of the target present trials. On 25% there was a feature change, on the remaining 25% there was a repetition. There were also eight *dimension context-25* blocks. These contained 25% dimension change trials, 25% feature change trials, and 50% repetitions. Block order was randomized across participants. Trial types within each block were randomly mixed.

Results and discussion

Trials with response times shorter than 200 ms and longer than 1500 ms (2.5% of the total numbers of trials) were not analysed. Overall, in this and

the following experiments, responses were faster and more accurate for colour targets than for orientation targets. In a similar vein, effects were often more pronounced for colour than for orientation trials. This higher effectiveness of colour has been reported several times before (e.g., Found & Müller, 1996; Müller et al., 2003; Olivers & Humphreys, 2003) and we will not discuss it further. For all experiments, we present the data collapsed across colour and orientation trials.

On average, observers made 2.2% errors on present trials, and 7.7% errors on absent trials, a difference that was significant, $t(17) = 3.92$, $p < .001$. The higher error rate on absent trials is somewhat unusual for visual search tasks, but here a bias towards "present" responses makes sense given that there were only 25% absent trials. Error rates for present trials are shown in Table 1. They largely followed the pattern of RTs, so we will concentrate on the latter.

Overall, participants responded faster on present than on absent trials (474 ms vs. 584 ms respectively), $t(17) = 10.55$, $p < .001$. Because we were interested in target changes, further analyses were confined to present trials following present trials. For these trials, we calculated the mean costs associated with a target change (whether a feature or a dimension change) relative to a target repetition, as a function of the different trial contexts. These switch costs are shown in Figure 1, together with the baseline (repetition) RTs relative to which they were calculated. An ANOVA with trial context as a factor (three levels) revealed that these baseline RTs increased with increasing proportions of dimension trials, $F(2, 34) = 11.96$, $MSE = 445.42$, $p < .001$.

An ANOVA on the dimension context conditions only, with context (25% or 50% dimension change trials), and change type (feature change, dimension change) as factors, revealed that dimension changes resulted in larger costs than feature changes $F(1, 17) = 29.03$, $MSE = 788.91$, $p < .001$, with no further interactions. Dimension change costs were greater than zero,

TABLE 1
Error percentages for the target present trials of Experiments 1 and 2

		Intertrial relationship		
Experiment	*Context*	*Repetition*	*Feature change*	*Dimension change*
1	Feature	1.3	1.4	
	Dimension-25	1.3	2.1	3.9
	Dimension-50	2.4	2.3	2.9
2	Feature	1.5	2.5	
	Dimension-25	1.0	0.8	6.7
	Dimension-50	0.9	1.3	5.4

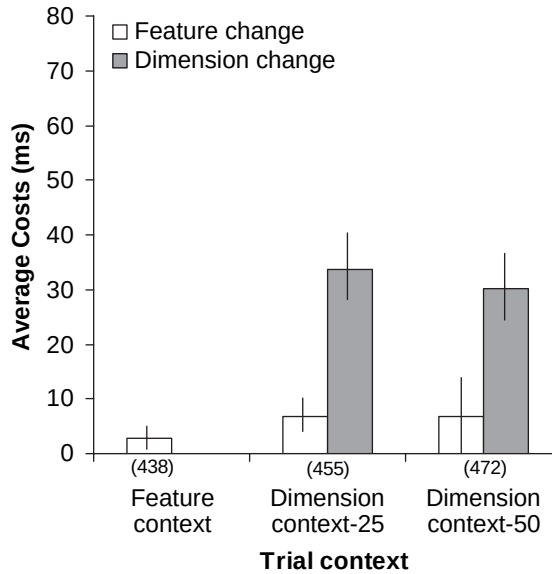

Figure 1. Average costs on target switch trials of Experiment 1, relative to target repetitions, and as a function of the trial context. Between parentheses are the RTs for the target repetition trials relative to which the costs have been calculated. Error bars denote one standard error of the mean cost.

$F(1, 17) = 43.97$, $MSE = 841.05$, $p < .001$, whereas feature change costs showed a trend in the same direction, $F(1, 17) = 3.40$, $MSE = 484.32$, $p = .083$.

The most important question was whether feature-based intertrial effects would be stronger in feature context blocks than in dimension context blocks. For this purpose, dimension changes were left out of the analyses, and we looked at feature changes only for the three different contexts (feature, dimension-25, and dimension-50). Across these contexts, feature change costs approached significance, $F(1, 17) = 4.04$, $MSE = 398.80$, $p = .061$. However, a one-way ANOVA revealed no sign of an effect of context, $F < 1$. Feature-based intertrial effects were no stronger within a feature context than within a dimension context. In all then, there was no evidence that the strength of feature-based intertrial effects depends on the type of change on surrounding trials.

EXPERIMENT 2

Experiment 1 failed to show an effect of trial context on intertrial priming effects. However, since we are reporting a null effect here, and intertrial priming effects were subtle in the first place, we decided that a replication

was desirable, preferably one based on more reliable intertrial priming effects. As demonstrated by Meeter and Olivers (2006), one way of strengthening intertrial effects is by increasing the display ambiguity. In Experiment 2, we adopted the same strategy: We made the displays noisier by randomly varying the size of the bars. Furthermore, instead of a relatively homogeneous grid that was completely filled with bars, we made the displays rather sparse, with a randomly filled grid and a random set size. The addition of noise was intended to introduce occasional salient distractor signals, and hence to make the displays more ambiguous. The prediction was that intertrial effects would become more reliable, and should thus allow for more subtle effects to emerge, such as those induced by trial context.

Method

Experiment 2 was the same as Experiment 1, except for the following changes: Eighteen observers participated (six female, mean age 20.3 years, range 17–30), all having self-reported normal eyesight and colour vision. Two additional observers took part but made more than 30% errors in one of the conditions. They were excluded from the analyses. The bars varied randomly in length between 1.7 and 2.5 degrees. Furthermore, the 11×9 virtual grid was now not completely filled, but on average each cell had a chance of 6.7% of being filled, leading to an average set size of 6.5, with a range of 1–18.

Results and discussion

Trials with response times shorter than 200 ms and longer than 1500 ms (2.9% of the total numbers of trials) were not analysed. On average, observers made 2.5% errors on present trials, and 9.0% errors on absent trials, a difference that was significant, $t(17) = 6.73$, $p < .001$. Error rates for present trials are shown in Table 1. Since the error pattern followed that of the RTs, we will concentrate on the RT analyses.

Overall, participants responded faster on present than on absent trials (500 ms vs. 634 ms respectively), $t(17) = 15.1$, $p < .001$. The following analyses were confined to present trials following present trials. For these trials, the costs associated with target changes are shown in Figure 2, together with the baseline repetition RTs. As in Experiment 1, these baseline RTs increased with increasing proportions of dimension trials, $F(2, 34) = 7.13$, $MSE = 392.28$, $p < .01$.

An ANOVA on the dimension context conditions only, with context (25% or 50% dimension change trials), and change type (feature change, dimension change) as factors, revealed that dimension changes resulted in

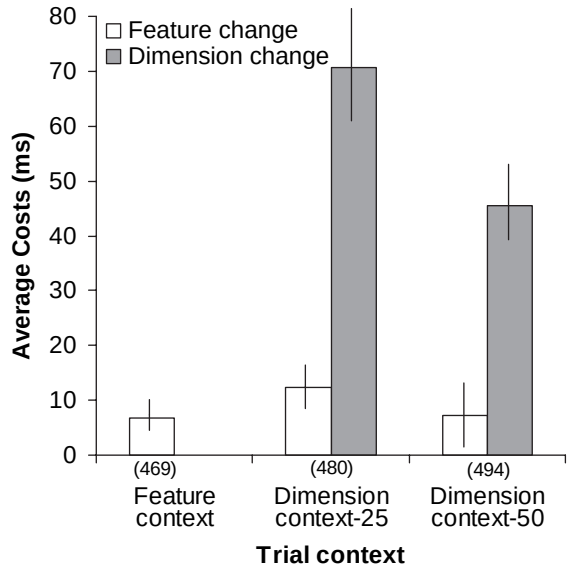

Figure 2. Average costs on target switch trials of Experiment 2, relative to target repetitions, and as a function of the trial context. Between parentheses are the RTs for the target repetition trials. Error bars denote one standard error of the mean cost.

larger costs than feature changes, $F(1, 17) = 51.13$, $MSE = 822.5$, $p < .001$, but this difference was larger in the dimension-25 than in the dimension-50 context, resulting in an interaction, $F(1, 17) = 4.91$, $MSE = 379.24$, $p < .05$. Across the two dimension contexts, dimension change costs were greater than zero, $F(1, 17) = 73.44$, $MSE = 1654.65$, $p < .001$, and the same was now true for feature changes, $F(1, 17) = 9.19$, $MSE = 374.59$, $p < .01$.

The most important question was whether feature-based intertrial effects would be stronger in feature context blocks than in dimension context blocks. For this purpose, dimension changes were left out of the analyses, and we looked at feature changes only for the three different contexts (feature, dimension-25, or dimension-50). Across these contexts, feature change costs were now reliably different from zero, $F(1, 17) = 18.61$, $MSE = 110.80$, $p < .001$. Nevertheless, a one-way ANOVA revealed again no sign of an effect of context, $F < 1$. Feature-based intertrial effects were no stronger within a feature context than within a dimension context.

EXPERIMENT 3

So far we have used a visual search task in which participants determined whether the target was present or absent. However, most of the studies that

found strong feature-based intertrial priming effects used compound search tasks, in which the target was always present, and observers made some decision about a specific target feature (e.g., Goolsby & Suzuki, 2001; Maljkovic & Nakayama, 1994; McPeek et al., 1999). In fact, to make matters more complicated, Olivers and Meeter (2006) found that intertrial effects in compound tasks tend to be rather weak or even absent, compared to present/absent tasks. It is only when there is a substantial level of ambiguity in the task that intertrial effects may re-emerge in compound tasks. Olivers and Meeter argued that, so far, the studies that have used compound tasks and have found strong intertrial priming effects, have indeed featured a considerable level of ambiguity.

Nevertheless, given the literature, the possibility still remains that in addition to ambiguity, the dimensional context also affects the strength of feature-based intertrial effects, but, for some reason, only in compound tasks. For example, one reason could be that there are no absent trials in compound tasks. The interspersing of targets with absent trials (as was the case in present/absent tasks of Experiments 1 and 2) may disrupt the optimization of weight settings, resulting in weaker effects even in the context of only feature changes. Experiment 3 therefore sought once more to address the potential influence of trial context on feature-based intertrial effects, but now in a compound search task. The search displays were the same as in Experiment 1, but without the target absent trials. Instead, participants responded to the pointing direction of an arrowhead inside the target bar. To bolster intertrial effects, we also included conditions in which a salient distractor was present (following Meeter & Olivers, 2006). The distractor was either colour defined (i.e., yellow) or orientation defined (i.e., horizontal), and could thus be defined on the same or on a different dimension as the target.

Method

The methods were the same as in Experiment 1, except for the following changes. There were eighteen participants (11 female, average age 22.6 years, range 18–35). An additional participant was left out of the analyses because of more than 30% errors in one of the conditions. There were no target absent trials. At the centre of each bar, a small (0.55×0.55 degrees) black arrow head was plotted that either pointed leftwards or rightwards. Participants were instructed to respond to the arrowhead in the target bar by pressing the "$<$" or "$>$" key, respectively, with the index and middle fingers of their preferred hand. There were three main conditions: There were two feature context conditions, one in which the target only changed colour (between red and green), one in which it only changed orientation

(between left- and right-tilted). In these blocks, there were 33% target repetitions and 67% target feature changes. There was one dimension context condition in which targets could change within dimensions (33% of the trials) and also between dimensions (33% of the trials; there were 33% repetitions). Participants practised 20 trials in each condition and then performed six blocks of 144 trials each (two blocks per condition) in completely counterbalanced order. Each block started with four warm-up trials that were not analysed. In total, the design yielded 32 trials per data cell.

Results and discussion

Error percentages are shown in Table 2. The error pattern does not go against that of the RTs, so we will focus on the latter.

Figure 3 shows the pattern of costs associated with the different target changes, as a function of trial context and distractor type, relative to the respective baseline (repetition) RTs. An initial ANOVA on these baseline RTs with trial context (feature, dimension) and distractor type (none, same dimension, different dimension) revealed that RTs were overall elevated in the dimension context, $F(1, 17) = 76.88$, $MSE = 793.02$, $p < .001$, and were elevated when a distractor was present, $F(2, 34) = 69.83$, $MSE = 814.11$, $p < .001$. There was also an interaction, $F(2, 34) = 18.87$, $MSE = 475.40$, $p < .001$, reflecting the fact that different dimension distractors were relatively more effective in the dimension context blocks than in the feature context blocks.

With regard to intertrial priming effects, across the different contexts, these were stronger when a distractor was present than when it was absent, $F(4, 68) = 4.42$, $MSE = 1413.20$, $p < .01$. Furthermore, within dimension

TABLE 2
Error percentages for Experiments 3 and 4

		Intertrial relationship		
Context	*Distractor type*	*Repetition*	*Feature change*	*Dimension change*
E3: Feature	None	3.6	3.2	
E3: Feature	Same dimension	3.1	3.5	
E3: Feature	Different dimension	3.0	3.2	
E3: Dimension	None	3.7	2.5	2.4
E3: Dimension	Same dimension	2.5	3.1	4.4
E3: Dimension	Different dimension	4.3	4.4	5.0
E4: Feature	None	2.4	3.1	
E4: Feature	Same dimension	3.0	3.9	
E4: Feature	Different dimension	3.7	4.0	

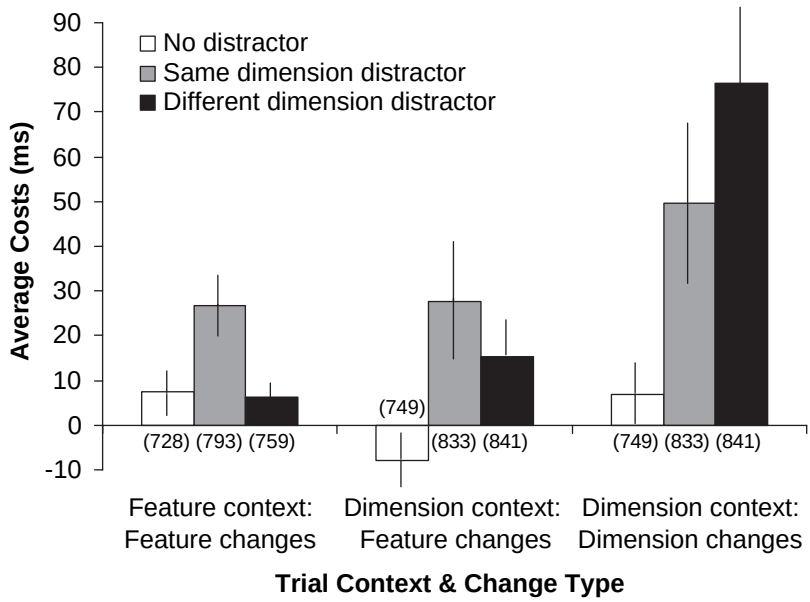

Figure 3. Average costs on target switch trials of Experiment 3, relative to target repetitions, as a function of the trial context and distractor type. Between parentheses are the RTs for the target repetition trials. Error bars denote one standard error of the mean cost.

context blocks only, different dimension distractors were more effective after a dimension change than after a feature change, $F(2,\ 34) = 3.68$, $MSE = 1547.43$, $p < .05$.

The most important question was whether feature change costs were modulated by the different trial contexts. On average, across contexts and distractor types, feature change costs were significantly greater than zero, $F(1,\ 17) = 87.01$, $MSE = 1057.00$, $p < .001$. However, an ANOVA with context and distractor type as factors revealed no effect of context ($F < 1$), nor a context by distractor type interaction, $F < 1.6$. The only factor that modulated feature costs was distractor type, $F(2,\ 34) = 36.63$, $MSE = 685.16$, $p < .001$. In line with Meeter and Olivers (2006), intertrial priming effects were greater when a distractor was present.

EXPERIMENT 4

So far, the experiments have failed to reveal an effect of the trial context on feature-based intertrial effects. However, before we can conclude that overall trial context plays no role in intertrial priming, we need to exclude one other possibility: There is a chance that the two different contexts allow for

different feature-based priming effects, but that this difference is overruled by observers applying the same overall attentional settings, or search mode, to both contexts. During the dimension change blocks, observers may adopt the strategy to only differentiate between targets on a global dimension level, and forget about feature differences. This strategy may then transfer to subsequent feature change blocks, even if feature differentiation might have been the more optimal strategy. Evidence for observers indeed sticking to a particular search strategy even if it is no longer the optimal one comes from Leber and Egeth (2006a, 2006b). They found that when observers adopt a certain search mode on the basis of a training session (be it singleton or feature search mode), this search mode is then also applied to a second session—even if the task has changed and the alternative search mode would be the more optimal choice.

A straightforward first step to assess such carry-over effects from dimension contexts to feature contexts would be to look at order effects. However, our experimental design was less than optimal for a systematic order analysis, and splitting up the data according to another factor would result in very few trials per cell (especially in Experiments 1 and 2, where we divided the experiment into 28 randomly ordered blocks). Furthermore, the initial practice trials always included dimension changes, so observers may have already established their dimension-based search strategy before the experiment proper started. We therefore chose a different strategy. In Experiment 4, a new set of observers was tested on the same visual search task as in Experiment 3, except that there were now never any dimension changes, only feature changes. Thus, performance could not be affected by carry-over effects from dimension-based search strategies. If observers indeed now adopt a more specific feature-based search mode, we should see feature-based intertrial priming effects increase relative to Experiment 3.

Method

Experiment 4 was the same as Experiment 3 except that (in addition to target repetitions) there were now only feature changes, no dimension changes. There were 12 participants (nine female, average age 21.3 years, range 18–32).

Results and discussion

Error percentages are shown in Table 2. The error pattern did not go against that of the RTs, so we will focus on the latter.

Figure 4 shows the pattern of costs associated with the feature changes, as a function of distractor type. For comparison, the equivalent conditions of

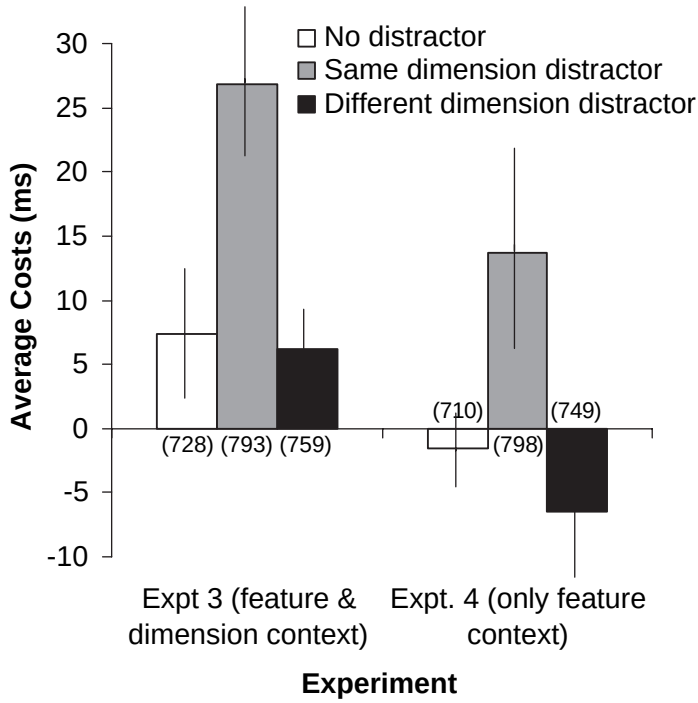

Figure 4. Average costs on target switch trials of Experiment 4 (in which there were only feature context blocks), as compared to Experiment 3 (in which there were also dimension context blocks).

Experiment 3 have also been plotted. An ANOVA on the baseline RTs of Experiment 4 revealed that RTs increased with the presence of a distractor, $F(2, 22) = 79.06$, $MSE = 298.24$, $p < .001$.

With regard to intertrial priming effects, feature change costs were strongest when a distractor of the same dimension was present, leading to a main effect of distractor type, $F(2, 22) = 3.77$, $MSE = 173.9$, $p < .05$. The important question was whether feature change costs were indeed increased when dimension context blocks were left out. Figure 4 suggests that this was not the case. If anything, effects were overall smaller in Experiment 4 than in Experiment 3. An ANOVA with experiment as a between-subjects factor indeed suggested a trend towards overall decreased effects, $F(1, 28) = 3.82$, $MSE = 1350.48$, $p = .061$. There were no interactions of experiment with other factors.

Finally, we look at response repetition effects. Some studies have suggested that intertrial priming effects for target-defining properties interact with the target's response property, such that costs associated with changes in the target-defining feature are particularly strong when the

response feature remains the same, whereas smaller costs are observed when both defining and response properties change together (e.g., Hommel, 1998; Müller & Krummenacher, 2006; Töllner, Gramann, Müller, Kiss, & Eimer, 2007). For this analysis, we used the feature context data of Experiments 3 and 4. The dimension context data of Experiment 3 were less suitable, because adding another factor (namely response relationship) resulted in fewer than 10 trials in some of the cells, making any estimates unreliable. Across Experiments 3 and 4, numerically, intertrial effects were indeed somewhat increased when the response repeated (11.6 ms) as compared to when it changed (7.4 ms), and this pattern was slightly more pronounced in Experiment 3 than in Experiment 4 (19.1 vs. 8.7 ms). However, statistically, none of the effects involving response relationship or experiment reached significance, all Fs < 2.1, all ps $> .15$. This does not mean that response repetition effects do not exist, but here they are simply not very strong.

GENERAL DISCUSSION

This study started from a pattern of results that appeared to emerge from the literature on intertrial effects in visual search. Some studies have found strong costs if the target changes feature from one trial to the next (notably Maljkovic & Nakayama, 1994, and follow-up studies), whereas others have found only weak feature-based effects, or no effects at all (notably Müller et al., 1995, and follow-up studies). We have recently identified one factor that contributes to such differential findings, namely the level of ambiguity in the visual search task (Meeter & Olivers, 2006; Olivers & Meeter, 2006). Here we investigated another factor that appeared to correlate with the division between weak and strong feature-based intertrial effects, namely the trial context. Strong feature-based effects have been found in tasks in which, when the target changed, it could only change within a dimension (i.e., in feature). Weak feature-based effects have been found when targets could also change between dimensions. Such context effects would make sense on the assumption that previous trials can alter the relative saliency and/or relevance of the current target feature.

The present study sought to systematically investigate the effect of trial context on feature-based intertrial costs by comparing feature changes within a feature context to those within a dimension context in the same group of participants, and within the same visual search tasks. However, there were no signs that feature-based intertrial effects depend on the trial context: They were no larger in blocks containing only feature changes than in blocks that also contained dimension changes—if anything, they were a little weaker.

We also tested for another possible confounding factor: The type of search task. Those studies that found strong feature-based intertrial effects tended to use compound search tasks rather than present/absent search tasks. However, we found no differences in the overall pattern of results between Experiments 1 and 2 (which used a present/absent task) and Experiment 3 (which used a compound search task). In neither task did trial context affect intertrial effects. Nor can the results be explained by the carry-over of a specific search strategy from dimension context blocks to feature context blocks. In Experiment 4, dimension context blocks were removed, without much effect on the feature change costs.

The results add further support to the idea that intertrial effects are largely automatic priming effects which are hardly susceptible to context, available resources, and explicit top-down knowledge-based processes (e.g., Maljkovic & Nakayama, 1994; Olivers & Humphreys, 2003; Pinto et al., 2005). In this respect, our results complement those of a recent study by Müller, Krummenacher, and Heller (2004). They asked observers to search for a target that could change in feature as well as dimension (between colour and orientation). They also instructed observers to explicitly remember the target that they had just found, as they would occasionally be asked for its identity. In one condition, observers merely had to remember the target-defining dimension (“colour” or “orientation”). In another condition, they had to remember the exact feature (“red”, “green”, “left-tilted”, or “right-tilted”). Müller et al. hypothesized that if intertrial effects are susceptible to explicit top-down control (in this case working memory requirements), then these effects should depend on the type of memory involved. Overall, RTs were increased in the memory conditions relative to a no memory control condition, and so were intertrial priming effects. However, even though participants were considerably slower in the feature memory condition than in the dimension memory condition, there were no differential effects of memory type on the magnitude of the intertrial effects.

This is not to say that intertrial priming effects reflect pure bottom-up processes. After all, intertrial priming does depend on the target description, and hence on the overall task settings. But it appears that, given certain task settings, intertrial priming effects occur rather automatically, without much on-line modulation (but see Müller et al., 2003; Theeuwes, Reimann, & Mortier, 2006, for recent debate). In this respect, these effects may be a prime example of what Bargh (1992) has referred to as conditional automaticity.

So far then, the factor that appears to be most capable of explaining the differences in magnitude of intertrial priming is the level of ambiguity (Meeter & Olivers, 2006; Olivers & Meeter, 2006). The present study offers further support for this. We increased display heterogeneity from Experiment 1 to Experiment 2 and found intertrial priming effects to become more reliable. In Experiment 3, we manipulated ambiguity by introducing a salient

distractor. Intertrial priming effects increased with the presence of such a distractor, especially when the distractor dimension became task relevant. As we have argued elsewhere (Meeter & Olivers, 2006; Olivers & Meeter, 2006), such differences probably also explain why some have found stronger intertrial effects than others—even strong feature-based effects (e.g., Maljkovic & Nakayama, 1994). We argue that increased ambiguity reflects increased levels of competition within a display, and hence for the need to apply stronger attentional weights to a particular target, with stronger intertrial effects as a result. Alternatively (or in addition), the competition may provide more room, or time, for intertrial priming to exert its effect—in other words, for intertrial effects to become measurable. Future research will need to address whether ambiguity causes intertrial priming, allows for its expression, or both. For now, we conclude that it does not depend on the general context of trials.

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